

# **For Reference**

---

**NOT TO BE TAKEN FROM THIS ROOM**

Ex LIBRIS  
UNIVERSITATIS  
ALBERTAENSIS













THE UNIVERSITY OF ALBERTA

RELEASE FORM

NAME OF AUTHOR .ANDRE NORMAND GAREAU.....  
TITLE OF THESIS .JUSTIFICATION AND LOCUS OF CONTROL IN VICTIMS  
.....  
.....  
DEGREE FOR WHICH THESIS WAS PRESENTED .MASTER OF ARTS.....  
YEAR THIS DEGREE GRANTED .....1973.....

Permission is hereby granted to THE UNIVERSITY OF  
ALBERTA LIBRARY to reproduce single copies of this  
thesis and to lend or sell such copies for private,  
scholarly or scientific research purposes only.

The author reserves other publication rights, and  
neither the thesis nor extensive extracts from it may  
be printed or otherwise reproduced without the author's  
written permission.





THE UNIVERSITY OF ALBERTA  
FACULTY OF GRADUATE STUDIES AND RESEARCH

The undersigned certify that they have read, and recommend to  
the faculty, JUSTIFICATION AND LOCUS OF CONTROL IN VICTIMS, a thesis  
entitled "Justification and Locus of Control in Victims" submitted by  
Andre Normand Gareau in partial fulfillment of the requirements for  
the degree of Master of Arts by



ANDRE NORMAND GAREAU

A THESIS  
SUBMITTED TO THE FACULTY OF GRADUATE STUDIES AND RESEARCH  
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE  
OF MASTER OF ARTS

DEPARTMENT OF PSYCHOLOGY  
EDMONTON, ALBERTA  
SPRING, 1973



THE UNIVERSITY OF ALBERTA  
FACULTY OF GRADUATE STUDIES AND RESEARCH

The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies and Research, for acceptance, a thesis entitled "Justification and Locus of Control in Victims" submitted by Andre Normand Gareau in partial fulfillment of the requirements for the degree of Master of Arts.





## ABSTRACT

A study was designed to test the relation between locus of control and justification in victims. In the context of equity theory, justifications were seen as certain types of estimates of the inputs and outcomes of the participants in an equitable or inequitable situation. Personal control (beliefs about the control that a person possesses), ideological control (beliefs about the control that others in society possess), and victimization were independently manipulated in a 2 X 2 X 2 factorial design. Justification responses of 64 male college students were obtained via an evaluation questionnaire presented after their being victimized in a number-estimation task. Personality scores on the Rotter (1966) Internal-External Control test were also obtained.

Despite the success of the personal control and victimization manipulations, the predicted effects were not obtained: justification was not greater under conditions of internal control, and victimization produced a reduction in justification. The hypotheses received marginal experimental support in that victims justified more under internal-personal control than under external-personal control. The correlation data confirmed the association between perceived internal control and justification: Ss internal on the I-E personality factors perceived they were more in control, and the latter also justified more.



## ACKNOWLEDGEMENTS

I would like to thank Dr. Brendan Rule for her interest, assistance, and encouragement in all stages of this research. I would also like to express my appreciation to Dr. Conrad Morrow; his thought-provoking seminars led to the initial ideas for this thesis.

Marilyn McAra's assistance in data-processing, and Merrilyn Greig's typing skills were very much appreciated.





## TABLE OF CONTENTS

	Page
ABSTRACT .....	iv
ACKNOWLEDGEMENTS .....	v
TABLE OF CONTENTS .....	vi
INDEX OF TABLES .....	vii
INTRODUCTION .....	1
METHOD .....	7
RESULTS .....	12
DISCUSSION .....	20
REFERENCES .....	25
APPENDICES .....	29



## INDEX OF TABLES

	Page
TABLE 1. Justification Questionnaire .....	10
TABLE 2. Mean Ratings of Expected Correct Responses .....	13
TABLE 3. Mean Ratings of Expected Correct Responses .....	14
TABLE 4. Mean Ratings of the Experimenter .....	16
TABLE 5. Correlation Matrix .....	19





## INTRODUCTION

During the past twenty years, social psychologists have shown an increasing interest in situations involving aggression, harm-doing, and injustice. Most of this research has focused on the determinants of aggression (Berkowitz, 1962, 1965, 1968, 1970; Buss, 1961; Feshbach, 1964, 1967; Goranson, 1970; Milgram, 1964, 1965). However, some of the more recent studies have focused on the consequences of an initial aggressive or unjust act. These studies have dealt primarily with the harm-doer's actions and perceptions following the act of doing harm to another (Berkowitz, 1962; Berscheid and Walster, 1967; Berscheid, Walster, and Barclay, 1969; Berscheid, Boye, and Walster, 1968; Lerner and Simmons, 1966; Macauley and Walster, 1971; Rawlings, 1968; Walster, Walster and Berscheid, in press). Although some theoretical formulations exist, (Adams, 1965; Walster, Walster and Berscheid, in press), the behavior and perceptions of the victim in a situation involving harm-doing or injustice have generated considerably less research (Leventhal and Bergman, 1969; Ross, Thibaut and Evenbeck, 1971; Hannah, 1972; Ross and McMillen, 1972).

The present study was designed to investigate the reactions of victims in an inequitable situation. An inequitable situation is defined as one in which a person perceives that not all participants have equal input-outcome ratios. This definition of inequity has been applied to various situations involving harm-doing and injustice (Adams, 1965).

Walster, Walster, and Berscheid (in press) have presented a model for predicting the reactions of a victim in an inequitable situation. They have suggested that when the alternatives of retribution and retaliation



are unavailable, a victim will tend to justify the inequity. The concept of justification is a complex one. Scott and Lyman (1968) describe a justification as "an account in which one accepts the responsibility for the act in question, but where one denies the pejorative quality associated with it." Generally, justifications involve certain types of estimates of the inputs and outcomes of the participants in an interaction.

Walster, Walster, and Berscheid (in press) have suggested several ways that victims can justify to maintain equity. The victim may believe that his exploitation brings him compensating benefits. He may console himself that in the long run the harm-doer will be punished as he deserves. He may convince himself that his exploiter actually deserves his greater benefits because he possesses additional inputs. Finally, he may convince himself that his inputs are so low that he deserves the lower outcomes. By using one or more of these rationalizations, the victim can persuade himself that both he and the harm-doer have equal input-outcome ratios. Each of these justifications was observed in Lane's (1962) field study of the political and ideological views of working-class men.

So far, scant empirical attention has been paid to the factors which determine justification. One possible factor is suggested by a common element that appears to underlie the above justifications; the belief that persons deserve and feel responsible for their outcomes. For example, Lane (1962) concluded that the major justification for income inequality revolved around the belief that "the upper class deserve to be upper, and the lower class deserve no better than they get." Such a notion of responsibility seems to imply a belief in internal control, defined by





Rotter (1966) as a person's belief that rewards follow from or are contingent upon his own behavior. Thus high justification appears to be related to conditions of internal locus of control.

In a recent study, Ross, Thibaut and Evenbeck (1971) used a contingency manipulation that was operationally similar to internal-external control, but did not find that it produced differences in justification. Ss in the contingency (internal) condition were told that their outcomes on a rope-pulling task were dependent on their skill; Ss in the non-contingency (external condition) were told before each rope-pull what their outcomes would be. Ross et al. (1971) attributed their lack of findings to the fact that the Ss may have felt that they worked just as hard in both conditions. If Ss did not control their outcomes in the external condition, it was through no fault of their own. Since both the internal and external groups may have felt the same degree of control over their performance, it is not surprising that there were no differences in justification. The present study differed from Ross et al.'s (1971) study in that it was concerned with the S's sense of control over his performance rather than his sense of control over his outcomes.

In addition to believing that they do or do not exert control over their own performance, individuals may also differ concerning beliefs about the control that others in society possess. Gurin, Gurin, Beattie and Lao (1969) referred to this distinction as ideological control (beliefs about the control that most others in the society possess) and personal control (beliefs about the control that one personally possesses). Research with Negro Americans has revealed that this distinction is a valid one: the



correlation between the personal and ideological factors is only .124 (Lao, 1970). Gurin et al. (1969) explained the existence of this distinction in the following way: the rationale is that Negroes may very well adopt the general beliefs about internal control (the general Protestant ethic that work is rewarded), but find that these beliefs cannot always be applied in their own life situations. Insofar as victims in general may share certain types of experiences with Gurin et al.'s (1969) Negro samples (discrimination, prejudice, reduced opportunities), the personal-ideological distinction may be relevant to the present investigations. Thus in this experiment both personal and ideological control were manipulated.

Personal control was varied by manipulating situational factors which signaled to the S that he was or was not in control of his performance. Ideological control was manipulated by influencing the S's beliefs concerning the control that persons in general have over their performance on the designated task. The major prediction was that both the ideologically and the personally internal Ss justify more than do externals.

Also of interest was the degree of justification which occurs under conflicting or incongruous conditions of personal and ideological control. It was expected that conflicting conditions would produce levels of justification intermediate to the non-conflicting conditions. A more specific hypothesis related to these conflicting conditions was advanced: ideological-internal, personal-external Ss may justify more than the ideological-external, personal-internal Ss. The basis for this prediction was derived from Gurin et al.'s (1969) observation that ideologically internal but personally external Negroes were less often involved in



social protest activity than were the more ideologically external Negroes. On the assumption that justification and social protest are inversely related (since justification involves acceptance of one's victimization), the ideologically internal, personally external Ss should justify more than the ideologically external, personally internal Ss. Furthermore, Scott and Lyman (1968) have emphasized that justifications are judged to be acceptable on the basis of commonly-held beliefs in particular cultures and situations. The implication for the present hypothesis is that justifications are more likely to be based on ideological beliefs than on personal beliefs. Thus a greater effect for ideological control than for internal control would stem from the fact that ideological control beliefs are more likely to be publicly-quoted, commonly-repeated, and difficult to challenge because they appear to represent the consensus of persons in society as a whole. On the other hand, personal control beliefs are more likely to be private, less likely to be validated by consensus, and thus more easily overcome by the group norms of ideological beliefs.

The major hypotheses were as follows:

1. Victimized Ss will justify more than non-victimized Ss.
2. Victims internal with respect to personal control will justify more than victims external with respect to personal control.
3. Victims internal with respect to ideological control will justify more than victims external with respect to ideological control.

The secondary hypotheses were:

4. Victims under conflicting conditions of personal and ideological control will justify at levels intermediate to victims under consistent





conditions of personal and ideological control.

5. Victims internal with respect to ideological control who are also external with respect to personal control will justify more than victims external with respect to ideological control who are also internal with respect to personal control.



## METHOD

### Subjects

The sample consisted of 64 male students from introductory psychology classes, who participated to earn 5 grade-points. The Ss signed appointment booklets which identified the experiment by a number and a nonsense name.

### Procedure

The Ss were individually presented with 40 slides each containing between 18 and 40 black dots. A tachistoscope controlled the time exposure of the slides. The Ss were instructed that they were participating in a study of perceptual judgments under various conditions, and the E stated that he had been hired to prepare and carry out the research project. (See Appendix A for the instructions given to the Ss.)

After stating that the experiment required relatively high motivation, the E offered the Ss 25 cents or 75 cents<sup>1</sup> for each estimate correct within 10% limits. Before making the actual (paid) judgments, however, the Ss were given two five-slide practice sessions: in the first practice session, the E showed the slides and called out the correct number of dots; in the second practice session, the Ss made five judgments and were told their "score." Then the rest of the slides were shown to the Ss.

Personal control manipulation. The Ss in the internal-personal

<sup>1</sup>Internal-personal Ss were offered 25 cents for each correct slide; external-personal Ss were offered 75 cents. Pilot work indicated that the different rewards were necessary to produce a uniform level of victimization or non-victimization without arousing suspicion.



condition (hereafter referred to as the internal-personal Ss)<sup>2</sup> were placed in a situation with cues to indicate that they had considerable control over their performance. The first set of practice slides was shown at 3 seconds. (Pilot work had indicated that this latter exposure time allowed Ss to correctly estimate about half the slides.) Following the second set of practice slides, the internal-personal Ss were informed that they had made 3 correct judgments out of 5, and that this indicated a significant skill factor, since the score was clearly above the chance level. Then the next 30 slides were shown at an exposure time of 3 seconds.

The external-personal Ss received feedback from the situation indicating that their performance was not within their control. The first set of practice slides was presented at 3 seconds, and the second set at 1/4 second. Following the second set, they were told that they had made only 1 correct judgment out of 5 and that this did not indicate the operation of a skill factor since a score of 1 out of 5 would be expected by chance alone. Then the remaining 30 slides were shown at an exposure time of 1/4 second.

Ideological control manipulation. Ss in the internal-ideological condition were told after the practice slides that according to cross-Canada samples on similar judgment tasks, most persons did significantly better than chance. Ss in the external-ideological condition were told that most persons performed at about the chance level.

<sup>2</sup>Note that terms such as internal-personals and external-ideologicals refer to Ss under the experimental conditions, and not to personality characteristics of the Ss.





Victimization manipulation. After the judgment trials were completed, the E told the Ss that they had made either 3 correct judgments at 75 cents each (external-personal Ss) or 9 correct judgments at 25 cents each (internal-personal Ss), which entitled them to \$2.25. To the Ss designated as victims, the E then announced that the money wasn't available, and that only certain persons received any money. Non-victims were told that the E couldn't give them exactly \$2.25, but that he could give them \$2.00. The non-victimized Ss then signed a receipt form and the E gave them the money.

The E then thanked all the Ss for their participation in the experiment, and told them that a detailed summary of the study would later be posted on the Psychology bulletin board. As he led the Ss out of the experiment room, the E waved to a pile of papers on a window sill and casually asked the Ss if they would mind filling out a rating form "required by the research supervisor", which was actually a questionnaire designed to measure justification. As this was an exploratory study, each of the justifications suggested by Walster, Walster, and Berscheid (in press) were investigated. Since the operationalization of justification is fairly complex, the questionnaire is presented in its entirety in Table 1. The rationale for the individual items is also presented.

The Ss were asked to deposit the justification questionnaire in a nearby padlocked box which was labelled with the research supervisor's name and various other experiment code names.

As the Ss began to fill out the questionnaire, the E left the area and secretly observed the Ss through a small window. After the Ss had



TABLE 1

## JUSTIFICATION QUESTIONNAIRE

NOTE: Low scores indicate more justification.

1. The learning experience provided by this experiment was:

considerable      1 2 3 4 5 6 7      very little

It was expected that victims and internals would justify by assigning more value to the learning experience. This would test the notion that victims may justify by believing that their exploitation brings them compensating benefits.

2. I think the experimenter's reasons for conducting the experiment as he did were:

very adequate      1 2 3 4 5 6 7      very inadequate

Victims were expected to find the E's reasons for conducting the study to be more adequate, since this would allow them to justify their lower outcomes (relative to the outcome of the E) by assigning higher inputs to the E.

3. Considering the education value of this experiment in comparison to the effort and skill required of me:

I learned quite a lot when you consider the small effort involved.      I learned very little for the large effort involved.

1 2 3 4 5 6 7

It was expected that victims and internals would justify their reduced outcomes by convincing themselves that they had invested very little effort in the task.

4. The experimenter seemed:

competent      1 2 3 4 5 6 7      incompetent

As in #2, it was expected that victims and internals would rate the E as more competent and thus justify their victimization on the basis of the E's higher inputs.

5. I think the Supervisor of Research will:

probably hire this experimenter again      probably not hire this experimenter again

7 6 5 4 3 2 1

It was expected that victims and internals would believe the E would not be rehired, and thus believe that justice would be attained in the long run when their harm-doer was punished. (See Appendix B for the actual form presented to S



completed the forms and were leaving the building, the E casually met the Ss in a corridor, and asked them to spare a few minutes to answer some questions about the experiment. These questions were intended to assess the Ss' perceptions of personal and ideological control in the experimental situation, and to assess the degree to which they felt victimized. (See Appendix C for these questionnaire items.)

After the Ss had completed the manipulation check questionnaire, the victimized Ss were given \$2.00. The E then proceeded with the post-experimental interview to check possible suspicion and to explain the true nature of the experiment (see Appendix D for a detailed presentation of the post-experimental interview). Following standardized debriefing procedures, it was emphasized that the experiment required the deception and that it had been difficult to achieve a plausible deception. After the interview the E thanked the Ss and stressed the importance of not discussing the experiment with classmates.

Throughout the experiment, an attempt was made to avoid experimenter effects by concealing the experimental conditions from the E. Only at the last possible moment, by secretly flipping a card (see Appendix A), did the E discover each of the conditions which the S would undergo.





## RESULTS

The results of six Ss<sup>3</sup> were discarded because of their suspicions of some part of the manipulations. (Appendix D contains the procedures used to determine suspicion). Two more Ss left the building before the end of the experiment. The results of the remaining 64 Ss were analyzed in a 2 X 2 X 2 factorial design, with 8 Ss in each cell.

The results of all the manipulation check items for victimization, personal control, and ideological control are reported below.

On a question asking Ss to rate their treatment on a 7-point scale from "very fairly" (1) to "very unfairly" (7), the victims indicated that they were treated less fairly than did the non-victims ( $F = 14.5$ ,  $df = 1/56$ ,  $p < .01$ ). The mean rating for the victims was 2.7; the mean rating for the non-victims was 1.5.

On three of the four questions used to measure personal control, Ss in the personal-internal condition expressed significantly more internal control than Ss in the external personal condition. On the first question, Ss were asked to rate their performance on a 7-point scale from "entirely a matter of skill" (1) to "entirely a matter of chance" (7). Compared to personal externals, personal internals felt that their performance was more a matter of skill ( $F = 6.61$ ,  $df = 1/56$ ,  $p < .025$ ). The mean rating for the personal-internals was 3.9; the mean rating for

<sup>3</sup>Of the six Ss who were discarded, four were non-victims and two were victims; four were in the personal-external condition, and five were in the ideological-external condition.



the personal-externals was 4.8. On the second question, Ss were asked to rate their confidence in their accuracy on a 7-point scale from "very confident" (1) to "not at all confident" (7). Personal internals reported that they were more confident of being correct ( $F = 6.95$ ,  $df = 1/56$ ,  $p < .025$ ). The mean rating for the personal-internals was 4.2; the mean rating for the personal-externals was 5.1. On the third question, Ss were asked to rate their accuracy on a 7-point scale from "close to 100% accurate" (1) to "close to 0% accurate" (7). Personal-internals reported that they were more accurate ( $F = 5.90$ ,  $df = 1/56$ ,  $p < .025$ ). The mean rating for the personal-internals was 4.3; the mean rating for the personal-externals was 5.1. On a fourth question, Ss were asked to rate whether or not concentration improved their performance, using a 7-point scale from "yes, definitely" (1) to "no, not at all" (7). There were no significant differences between personal-internals and personal-externals.

Ss' responses on the question concerning the expected percentage of correct responses produced a significant interaction between personal control and victimization ( $F = 5.88$ ,  $df = 1/56$ ,  $p < .025$ ; see Table 2).

Table 2

## MEAN RATINGS OF EXPECTED CORRECT RESPONSES

		Victimization Condition	
		Victims	Non-victims
Personal Control Condition	Internals	4.8*	3.9
	Externals	4.8	5.4

\*A 7-point scale from "close to 100% accurate" (1) to "close to 0% accurate" (7) was used.



Inspection of this interaction indicated that under internal-personal control, victims rather than non-victims perceived that they were less accurate; under external-personal conditions, the non-victims rather than victims perceived that they were less accurate. Examination of the means also indicated that for victims, there were no differences between personal-internals and personal-externals; for non-victims, however, personal-internals felt they were more accurate than did personal-externals ( $t = 3.46$ ,  $df = 30$ ,  $p < .002$ ).

Responses to the question about perceived accuracy also produced a significant interaction between personal and ideological control ( $F = 4.08$ ,  $df = 1/56$ ,  $p < .05$ ).

Table 3

## MEAN RATINGS OF EXPECTED CORRECT RESPONSES

		Personal Control Condition	
		Internal	External
Ideological Control Condition	Internal	4.2*	5.6
	External	4.5	4.6

\*A 7-point scale from "close to 100% accurate" (1) to "close to 0% accurate" (7) was used.

The cross-over interaction presented in Table 3 indicated that under internal-personal control, ideological-internals compared to ideological-externals perceived that they were more accurate; under external-personal conditions, however, the ideological-externals compared to ideological-internals perceived that they were more accurate. Examination of the means also indicated that under ideological-external conditions, the difference between personal-internals and personal-externals was not





significant; but under ideological-internal conditions, personal-internals felt they were more accurate than did personal-externals ( $t = 3.40$ ,  $df = 30$ ,  $p < .002$ ).

Ss were also asked to rate the performance of "most people" on a 7-point scale from "entirely a matter of chance" (1) to "entirely a matter of skill" (7). The differences between ideological-internals and ideological-externals on this manipulation check for ideological control did not approach significance.

Justification effects were measured by responses on 7-point scales to five questionnaire items. In addition, a summed justification score was obtained for each S. (Throughout the following discussion, lower scores indicate more justification.)

Contrary to the hypothesis, victims showed less justification than non-victims on the summed measure ( $F = 5.49$ ,  $df = 1/56$ ,  $p < .025$ ). The mean rating for the victims was 18.9; the mean rating for the non-victims was 16.8. Victims also showed less justification on two of the five individual justification questions. On one question, Ss were asked to rate the E's reasons for conducting the experiment as he did on a scale from "very adequate" (1) to "completely inadequate" (7). Compared to non-victims, victims thought that the E's explanations for the experiment were less adequate ( $F = 6.94$ ,  $df = 1/56$ ,  $p < .025$ ). The mean rating for the victims was 3.6 and the mean rating for the non-victims was 2.6. Also, when asked to rate the E's competence on a scale from "competent" (1) to "incompetent" (7), victims rated the E as less competent than did non-victims ( $F = 6.52$ ,  $df = 1/56$ ,  $p < .025$ ). The mean for victims was 2.2;



the mean rating for non-victims was 1.5.

On the question dealing with the E's competence, there was an interaction between victimization and personal control ( $F = 4.36$ ,  $df = 1/56$ ,  $p < .05$ ; see Table 4).

Table 4  
MEAN RATINGS OF THE EXPERIMENTER

		Personal Control Level	
		Internal	External
Victimization	Victims	1.9*	2.5
	Non-Victims	1.8	1.2

\*A 7-point scale from "competent" (1) to "incompetent" (7) was used.

For victims, Ss under external control conditions judged the E less competent than Ss under internal control; for non-victims, externally-controlled Ss judged the E as more competent than Ss under internal control. A t-test indicated that neither of these two differences was significant.

The justification data indicated that neither the personal control nor the ideological control main effects were significant.

### Correlational analysis

In addition to the analyses of variance, correlational analyses were done in order to clarify the nature of the relationships, and to compare the justification and manipulation check data with the personality test scores of the Ss on Rotter's Internal-External (I-E) control test (Rotter, 1966). Since Rotter's scale contains several sub-scales, these were also included in the analyses. The subscales or factors used closely



correspond to Gurin et al.'s (1969) personal and ideological factors, and to Mirel's (1970) political factor. (See Appendix G for a listing of the test items involved on each factor.)

I-E scores and factor scores were available for 34 of the 64 Ss.<sup>4</sup> The means of the manipulation check data and the justification data were very similar for this group of 34 and the complete sample of 64 Ss; t-tests revealed no significant differences between the means for any item. (See Appendix H for the presentation of means for the two groups.)

The complete correlation matrix is presented in Table 5 (p. 19). Items #1-11 refer to the manipulation check and justification questions listed in Appendices B and C; item #12 is the summed justification measure; and items #13-16 represent the Rotter I-E scale score, I-E personal control factor, I-E political control factor, and I-E ideological control factor, respectively.

The significance of the correlations was tested using a two-tailed t-ratio suggested by Hays (1963, p. 529).<sup>5</sup> The following relationships were obtained:

<sup>4</sup>The Ss had taken the test as part of a mass testing program six months prior to the experiment.

<sup>5</sup>Of the 120 correlations presented in Table 5, only 69 are relevant to this investigation. It is acknowledged that with such a large number of correlations, chance factors alone would lead one to expect approximately 3 correlations significant at the .05 level. In this analysis, 9 significant correlations were obtained, some of these at the .01 level. Also, all of the 9 significant correlations are consistent with the hypotheses.



- (a) Justification and I-E scores. Ss external on the ideological factor of the Rotter I-E scale believed that the learning experience provided by the experiment was lesser ( $r = .49, p < .01$ ), and showed less overall justification ( $r = .38, p < .05$ ) than Ss internal on the ideological factor of the I-E.
- (b) Perceived internal-external control and I-E scores. Ss' confidence in being correct was significantly associated with greater internality on the I-E scale ( $r = .40, p < .05$ ), on the personal I-E factor ( $r = .39, p < .05$ ) and on the ideological I-E factor ( $r = .35, p < .05$ ).
- (c) Justification and perceived internal-external control in the experiment. All of the significant associations between these two variables indicated that perception of internal control in the experimental situation was associated with greater justification on the summed measure. Greater justification was associated with belief that the perceptual judgments constituted a skill rather than a chance task ( $r = .34, p < .05$ ), with greater confidence of being correct ( $r = .41, p < .02$ ), with perception that concentration improves one's performance ( $r = .40, p < .02$ ), and with belief that the performance of most people would be a matter of skill ( $r = .36, p < .05$ ).





Table 5  
CORRELATION MATRIX<sup>1,2</sup>

CORRELATION MATRIX																	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	
	high-low % correct estimated	low-high concentration effect	skill-chance task	confident-not confident	fair-unfair treatment	chance-skill for most	people	considerable-little learning	adequate-inadequate reasons	small-large effort	competent-incompetent E	hire-not hire E	high-low summed I-E	Rotter I-E	I-E (Personal)	I-E (Political)	I-E (Ideological)
1																	
2	10																
3	33	-30															
4	71	-09	42														
5	01	17	-09	17													
6	-09	18	-48	00	-14												
7	35	-41	36	34	-02	33											
8	25	-30	02	21	10	24	20										
9	06	-28	37	26	22	32	41	10									
10	02	08	-05	04	47	23	10	44	21								
11	-03	03	-10	-14	20	23	18	10	00	47							
12	32	-40*	34*	41*	18	36*	69	62	64	42	-06						
13	32	-11	14	40*	13	-06	28	17	15	05	-04	30					
14	34	-07	18	39*	-03	-09	15	00	10	-09	-04	10	86				
15	12	-17	07	23	21	-09	09	32	06	-18	-04	26	65	42			
16	37	-08	17	35*	23	02	49**	11	23	-04	-05	38*	71	57	25		

\*p < .05

\*\*p < .01

<sup>1</sup>Note that significant correlations which were irrelevant to the analysis were not marked with asterisks.

<sup>2</sup>For each measure, the designation of the response alternative on the left was scored low.



## DISCUSSION

Contrary to expectations, victims justified less than non-victims. Two explanations may account for this finding. First, there may be some question concerning the actual degree of victimization felt by the victims. The manipulation check showed that victims rated their treatment as significantly less fair than did non-victims; however, the victims still rated their treatment on the positive side (Mean = 2.7) of a 7-point "fair-unfair" scale. On the other hand, the victimized Ss' frequent and obvious signs of dismay (and the behavior of the two Ss who were too upset to continue) suggested to the E that the victims truly felt somewhat victimized. It appears that any rating less than "very fair" indicates that some victimization was felt.

A more plausible explanation for the failure of the victims to show greater justification may be that non-victims were not just non-victimized; they were given a \$2.00 reward for twenty minutes of experimental participation. This was probably not a very common experience for the Ss, since they were receiving grade-credits for their participation. It is thus possible that non-victims may have felt over-rewarded and may have been influenced to restore equity by giving the E and the experiment an especially high rating (i.e., very high justification).

Although there were no significant main effects for the personal and ideological control variables, the interaction data indicated that victimization affected the Ss' perceptions of personal control: non-victimized personal-internals felt they were estimating more correctly than did externals, but for the victimized Ss there were no differences



between personal-internals and personal-externals. Possibly the experience of feeling victimized was somewhat overpowering and may have distracted the Ss from perceiving the actual locus of control in the situation.

On the same question dealing with the perceived accuracy of the Ss' estimates, there was an interaction between personal and ideological control. Under ideological-internal control, personal-internals felt they were estimating more correctly than did personal-externals; there were no such differences for Ss under ideological-external control. A perceptual set may have been operating. Once Ss are given cues that success on the task is a matter of chance for most persons, they may be less likely to attend to cues relating to their personal control in that situation.

On the justification item dealing with the Ss' judgments of the E's competence, there was an interesting interaction between victimization and personal control. As expected, victimized Ss justified more under conditions of internal-personal control than under conditions of external-personal control. Non-victims showed the opposite trend, in that non-victimized personal-internals justified less than did the non-victimized personal-externals. This latter finding may be due to the non-victims having been induced by their relatively large reward, to return a favor to the E. Those non-victims most likely to feel this pressure toward restoring equity would be those under external control, who would be less likely to feel that they had earned the reward. Thus the non-victimized external-personals may have had a greater need to justify than did the non-victimized internal-personals.





It should be noted that the personal control manipulation was not totally unambiguous. A careful attempt was made to demonstrate to the Ss that their results were due to skill or chance, and the manipulation checks indicated that the manipulation was successful. However, it is possible that in both conditions, Ss may have perceived that it was the E who was really in control, since the slide exposure time was preselected. Thus the lack of significant justification effects may be due to the lack of very clear differences between the two personal control conditions.

Examination of the correlation data suggests that the relative impotence of the experimental manipulations may have been caused by individual differences in internal-external control. Ss who were more internal on both the personal and ideological factors<sup>6</sup> of the Rotter I-E test were more likely to see themselves in control. Also, greater justification was associated with perceived internal control: Ss' perceptions that they were in control accounted for between 12% and 15% of the variation in justification scores.

Of the four I-E personality measures used, only the I-E ideological factor was significantly associated with any of the justification measures. The I-E political factor was neither associated with perception of internal control, nor was it associated with justification.

This study has several implications for understanding reactions of victims, and locus of control. In comparison to non-victims, victims in general do not make greater use of the justification techniques

<sup>6</sup>Although Lao (1970) had reported a correlation of only .124 between personal and ideological control for her sample of Negro Americans, the present sample of freshman students yielded a correlation of .567.



described by Walster et al. (in press): belief that the victimization is deserved, belief that the harm-doer will be punished in the long run, etc. In fact, victims tend to justify less than non-victims: the present study thus confirms Ross et al.'s (1971) finding that victims tend to derogate the person and the situation responsible for their victimization.

In the present study, however, it is possible that the greater justification shown by non-victims may be due to a special combination of circumstances. Two modifications of the experimental situation can be suggested to test the hypothesis that victims justify more than non-victims. First, the possibility that non-victims might feel over-rewarded should be eliminated. Secondly, the Ss' opportunities to retaliate should be blocked more completely. This is necessary because according to the formulations of Walster et al. (in press), victims are expected to justify only when the avenues of restitution and retaliation are unavailable. In the present experiment, victims may have perceived the experiment evaluation as an opportunity to retaliate by giving a low rating to the E and to the experiment.

One might question why victims should justify at all, if they are in a situation where it is quite clear that they are not responsible for their outcomes. It is generally accepted that responsibility is an important variable in justification, theoretically and empirically. Scott and Lyman (1968) categorize justification as a type of "account" given when the S feels responsible for his action; if he does not feel responsible, the "account" falls in the category of excuses. Various



studies have documented the effect of responsibility on the reactions of harm-doers (Lerner and Matthews, 1967; Regan, 1971; Thalhoffer, 1971). Depending on the circumstances, feelings of responsibility may lead to guilt and restitution, or to rejection and devaluation of the victim in an attempt to justify the situation. However, it has not yet been shown that victims need to be responsible in order to justify. Walster et al. (in press) imply that even though victims may not initially feel responsible for their outcomes, the need to believe in a just world may lead them to justify. This process is similar to that suggested by Lerner and Matthews' (1967) analysis of harm-doers whose belief in a just world may have led them to devalue their victim. It is thus conceivable that victimization even without actual responsibility could result in justification in victims.

It will be recalled that this study was inspired by Lane's (1962) observations that feelings of responsibility (internal control) might be a major support or "cause" of justification. Due to the lack of experimental verification, there is little to support such a cause-effect relationship, although the association between justification and perceived internal control was confirmed. More conclusive answers await further research, as various explanations can be offered for the lack of experimental effects.



## References

- Adams, J. S. Inequity in social exchange. In Berkowitz, L. (Ed.) Advances in Experimental Social Psychology, Vol. II. New York: Academic Press, 1965, pp. 267-299.
- Aronson, E., and Carlsmith, J.M. Experimentation in social psychology. In Lindzey, G. and Aronson, E. (Eds.) The Handbook of Social Psychology. Reading, Mass.: Addison-Wesley, 1968, Vol. 2, 1-80.
- Berkowitz, L. Aggression: A social psychological analysis. New York: McGraw-Hill, 1962.
- Berkowitz, L. The concept of aggressive drive: some additional considerations. In Berkowitz, L. (Ed.) Advances in Experimental Social Psychology, Vol. II. New York: Academic Press, 1965, pp. 301-329.
- Berkowitz, L. Roots of aggression: a re-examination of the frustration-aggression hypothesis. New York: Atherton, 1969.
- Berkowitz, L. Experimental investigations of hostility catharsis. Journal of Consulting and Clinical Psychology, 1970, 35(1), 1-7.
- Berscheid, E., Boye, D., and Walster, E. Retaliation as a means of restoring equity. Journal of Personality and Social Psychology, 1968, 10, 370-376.
- Berscheid, E., and Walster, E. When does a harm-doer compensate a victim? Journal of Personality and Social Psychology, 1967, 6, 435-441.
- Berscheid, E., Walster, E., and Barclay, A. Effects of time on tendency to compensate a victim. Psychological Reports, 1969, 25, 431-436.
- Buss, A. H. The psychology of aggression. New York: John Wiley, 1961.





- Feshbach, S. The function of aggression and the regulation of aggressive drive. Psychological Review, 1964, 71, 257-272.
- Feshbach, S. The reinforcing effects of witnessing aggression. Journal of Experimental Research in Personality, 1967, 2, 133-139.
- Goranson, R. E. Media violence and aggressive behavior: a review of experimental research. In Berkowitz, L. (Ed.) Advances in Experimental Social Psychology, Vol. V. New York: Academic Press, 1970, pp. 2-31.
- Gurin, P., Gurin, G., Lao, R. C., and Beattie, M. Internal-external control in the motivational dynamics of Negro youth. Journal of Social Issues, 1969, 25, 29-53.
- Hannah, T. E. The behavioral consequences of arbitrary discrimination. Paper presented at the 1972 convention of the Canadian Psychological Association.
- Hays, W. L. Statistics for psychologists. New York: Holt, Rinehart, and Winston, 1963.
- Lane, R. E. Political ideology. New York: The Free Press, MacMillan Co., 1962.
- Lao, R. E. Internal-external control and competent and innovative behavior among Negro college students. Journal of Personality and Social Psychology, 1970, 14, 263-270.
- Lerner, M. J., and Matthews, G. Reaction to suffering of others under conditions of indirect responsibility. Journal of Personality and Social Psychology, 1967, 5, 319-325.
- Lerner, M. J., and Simmons, C. H. Observers' reactions to the "innocent" victim. Compassion or rejection? Journal of Personality and Social Psychology, 1966, 4, 203-210.



- Macauley, S., and Walster, E. Legal structures and restoring equity. Journal of Social Issues, 1971, 27(2), 173-188.
- Milgram, S. Group pressure and action against a person. Journal of Abnormal and Social Psychology, 1964, 69, 137-143.
- Milgram, S. Some conditions of obedience and disobedience to authority. Human Relations, 1965, 18, 57-76.
- Mirels, H. L. Dimensions of internal versus external control. Journal of Consulting and Clinical Psychology, 1970, 34, 226-228.
- Rawlings, E. Witnessing harm to others: a reassessment of the role of guilt in altruistic behavior. Journal of Personality and Social Psychology, 1968, 10, 377-380.
- Regan, J. W. Guilt, perceived injustice, and altruistic behavior. Journal of Personality and Social Psychology, 1971, 18, 124-132.
- Rotter, J. B. Generalized expectancies for internal versus external control of reinforcement. Psychological Monographs, 1966, 80, 1-28.
- Ross, M., and McMillen, M. The effect of external referents and past outcomes on satisfaction and retaliation. Paper presented at the 1972 convention of the Canadian Psychological Association.
- Ross, M., Thibaut, J., and Evenbeck, S. Some determinants of the intensity of social protest. Journal of Experimental Social Psychology, 1971, 7, 401-418.
- Scott, M. B., and Lyman, S. M. Accounts. American Sociological Review, 1968, 33, 46-62.
- Thalhofer, N. N. Responsibility, reparation, and self-protection as reasons for three types of helping. Journal of Personality and Social Psychology, 1971, 19, 144-151.



- Walster, E., Berscheid, E., and Walster, G. W. The exploited: Justice or justification? In Macauley, M. R., and Berkowitz, L. (Eds.), Altruism and Helping Behavior. New York: Academic Press, 1970.
- Walster, E., Walster, G. W., and Berscheid, E. New directions in equity research. In Berkowitz, L. (Ed.) Advances in Experimental Social Psychology (in press).





APPENDIX A  
INSTRUCTIONS TO SUBJECTS

The E greeted the S and asked him to sit down. (At this point the E flipped a card which determined the level of personal control for the S.) Then the E said:

"This experiment is part of a larger research project to study judgments of numbers. This particular experiment is just one of a series designed to study the perceptual judgments of persons under several kinds of conditions. I've been hired as an experimenter responsible for preparing and carrying out this part of the research. In this experiment, you'll be asked to make estimates concerning the number of dots on a series of slides. Each slide has a number of dots on it which you must estimate. Now, since this particular experiment is designed to study judgments made under relatively high motivation, I've arranged through the university grant funds for enough money to pay you 25¢ (personal-internals) or 75¢ (personal-externals) for each estimate which is correct within 10% limits. For example, if there are 10 dots and you estimate 9 or 11 dots, that would be correct.

Before we start the actual trials, you'll have 2 sets of practice trials of 5 slides each. For the first set, you'll just watch the slides while I call out the correct number of dots for each slide. Then, for the next set of 5 slides, you'll call out your estimate, and after that 5-slide set I'll tell you if you had any correct, and how many."

For external-personal Ss, the E then showed the first set of 5 slides



at 3 seconds exposure time, and the next 5 slides at 1/4 second. The E then said: "Your scores show you have one correct judgment out of 5. There is no evidence of a skill factor here; one out of 5 is about the level you would obtain by chance."

For internal-personal Ss, the E then showed the first 5 slides at 1/4 second exposure time, and the next 5 slides at 3 seconds. After the latter set the E said: "Your scores show you have made 3 correct judgments out of 5. This indicates the effect of a significant skill factor, since a score of 3 out of 5 is clearly above the chance level."

(At this point the E flipped a card to determine the level of ideological control for the S.) To the external-ideological Ss he said: "You might be interested to know that most persons tested so far have made correct judgments at about the level of chance. We have cross-Canada samples on this type of judgment task, and most persons perform at about the level of chance."

To the internal-ideological Ss the E said: "You might be interested to know that most persons tested so far have made correct judgments above the level of chance. We have cross-Canada samples on this type of judgment task, and most persons do significantly better than chance."

The E then showed the next 30 slides, at 3 seconds for the personal-internals, and 1/4 second for the personal-externals.

(After the slide presentation was over, the E secretly flipped a card to determine the level of victimization for the S.) To the victims, the E said: "You have 9 correct (personal internals) or 3 correct (personal externals). Let's see...that would be \$2.25. However, the



money isn't available. Only certain persons received any reward money."

To the non-victims, the E said: "You have 9 correct (personal-internals) or 3 correct (personal-externals). Could you show me your experiment card and I'll write out a receipt for that? I can't give you exactly \$2.25 but I can give you \$2.00." The E then gave \$2.00 to the non-victims.

To all Ss, the E said: "Well, that's all for the experiment. I can't explain any more about the experiment just now. The results and a full explanation of the experiment will be posted at the experiment sign-up table on April 6, the last day of classes. O.K.? Well, I'll sign your experiment card and that will be all. Thank you for coming to the experiment."

The E accompanied the S to the door, and then motioned to a pile of papers on a window sill in the corridor. He said casually, "Oh, on your way out, could you fill in an experiment rating form and drop it in the box in the hall? The supervisor of the research project wants an evaluation of the experiment."

As the S began to fill out the justification questionnaire (see Appendix B), the E left the area and secretly observed him through a small window. As the Ss was walking out of the building, the E "happened" to meet the S and said "Hi! Oh, by the way, would you have a few minutes to discuss the experiment? I'd like to have your impressions."

The E then led the S back to the experiment room and asked him to fill out the manipulation check questionnaire (see Appendix C). Finally, the E completed the post experimental interview to assess the S's



suspicious and to explain the purposes of the experiment (see Appendix D for the post-experimental interview).





APPENDIX B

EXPERIMENT ASSESSMENT QUESTIONNAIRE

Instructions: In order to assist the Supervisor of Research in evaluating this experiment, please answer the following questions as accurately as possible. Check (✓) the blank which most closely approximates your own feelings about the questions.

1. The learning experience provided by this experiment was:

considerable      1   2   3   4   5   6   7      very little      D.V. #7\*

2. I think the experimenter's reasons for conducting the experiment as he did were:

very  
adequate      1   2   3   4   5   6   7      very  
inadequate      D.V. #8

3. Considering the education value of this experiment in comparison to the effort and skill required of me,

I learned quite  
a lot when you  
consider the small  
effort involved.

1   2   3   4   5   6   7

I learned very  
little for the  
large amount of  
effort involved.

D.V. #9

4. The experimenter seemed:

competent      1   2   3   4   5   6   7      incompetent      D.V. #10

5. I think the Supervisor of Research will:

probably hire  
this experimenter  
again

1   2   3   4   5   6   7

probably not  
hire this  
experimenter again

D.V. #11

\*This number refers to the item No. in Table 5 (Correlation Matrix).



6. Comments: \_\_\_\_\_  
\_\_\_\_\_  
\_\_\_\_\_  
\_\_\_\_\_

7. What do you think is the purpose of this experiment? \_\_\_\_\_  
\_\_\_\_\_



## ZETA #51

## POST-EXPERIMENTAL QUESTIONNAIRE

To the Subject: Since the number-estimating task is fresh in your mind, we would like to know something about your personal impressions and feelings about it. Mark an "X" in the box of your choice.

- 1.\* During the judgment trials of this experiment, I felt that I was estimating correctly:

close to 100% of the time								close to 0% of the time
------------------------------	--	--	--	--	--	--	--	----------------------------

2. I felt that when I concentrated on the slide, I could usually do better in estimating the number of dots.

[illegible]

3. I believe my performance on this task was:

entirely a matter of skill								entirely a matter of chance
-------------------------------	--	--	--	--	--	--	--	--------------------------------

4. Concerning most of the estimates I made in the perceptual task, I felt:

very confident ☐ ☐ ☐ ☐ ☐ ☐ ☐ not at all  
of being correct confident of  
being correct

5. How do you feel you have been treated in this experiment?

[illegible]

6. The performance of most people on this perceptual task would be:

entirely a matter of chance								entirely a matter of skill
--------------------------------	--	--	--	--	--	--	--	-------------------------------

\*This number refers to the item No. in Table 5 (Correlation Matrix).





APPENDIX D  
POST-EXPERIMENTAL INTERVIEW<sup>1</sup>

After the S had completed the manipulation check questionnaire, the E said: "Now, I'd like to ask you a few questions to get a more detailed view of your impressions of the experiment.

1. First of all, were all the instructions clear?
2. Were all the purposes and procedures of the experiment clear to you?
3. Do you have any questions about the experiment?
4. People may react to experiments in different ways; could you comment on your impressions of the experiment, your strategies, or how you felt?"

If at this point the S indicated that he was aware of the purpose of the experiment, or if he indicated suspicion of the reward or of the instructions, then he was not included in the data sample. The following questions were leading questions designed to allow the S to show that he was not completely naive, and thus save face:

5. "Did you notice anything confusing or strange about the experiment?"
6. "Do you think there might have been more to the experiment than what you were told?"

The E then said: "Well, you're on the right track; it's true I was interested in some things I didn't tell you about."

Then the E discussed the problems of research involving various

<sup>1</sup>These procedures are largely based on suggestions by Aronson and Carlsmith (1968).



psychological reactions, and gradually explained to the S why it was necessary to distract his attention from the real purpose of the experiment in order to obtain a spontaneous, natural sample of behavior. Then the E briefly reviewed the notions of equity, justification, and locus of control; the true purposes of the experiment were revealed.

The E assured the S that it had not been easy to achieve a plausible deception, and explained the pilot work which had been required. The S was asked if he had any suggestions for improving the experiment, and was asked not to discuss the experiment with his classmates. When all the S's doubts and questions had been answered, the E thanked him for his help. To further insure their goodwill, Ss who were in the victimized condition were given the \$2.00 reward money.



## APPENDIX E

Scores for the personal, political, and ideological factors were obtained using the following items; item numbers refer to the Rotter (1966) test of Internal-External Control (see Appendix F for these test items).

1. Personal factor: #6, 10, 11, 12, 15, 17, 18, 20, 26, 28, 32.
2. Political factor: #4, 13, 19, 25, 33.
3. Ideological factor: #3, 7, 8, 18, 22, 24, 30.



## APPENDIX F

### I-E SCALE: ROTTER (1966)

Name \_\_\_\_\_

I. D. Number \_\_\_\_\_ Sex \_\_\_\_\_

### ATTITUDE QUESTIONNAIRE

We are interested in what people think about a number of social questions. The best answer to each statement is your personal opinion. The items are given in pairs. You are to choose one response from each pair. Choose the response that most closely fits your opinion or feeling and indicate your choice by blacking "A" or "B" corresponding to the response chosen. You indicate which response you agree with by blacking in the identifying letter on the IBM sheet. If you agree with both, choose the one you agree with most strongly. If you do not agree with either, choose the one you find the least disagreeable of the two.





---

A	B
1. Children get into trouble because their parents punish them too much.	The trouble with most children nowadays is that their parents are too easy with them.

---

A	B
2. Without the right breaks, one cannot be an effective leader.	Getting people to do the right thing depends upon ability; luck has little or nothing to do with it.

---

A	B
3. Many of the unhappy things in people's lives are partly due to bad luck.	People's misfortunes result from the mistakes they make.

---

A	B
4. One of the major reasons why we have wars is because people don't take enough interest in politics.	There will always be wars, no matter how hard people try to prevent them.

---

A	B
5. In the long run people get the respect they deserve in this world.	Unfortunately, an individual's worth often passes unrecognized no matter how hard he tries.

---

A	B
6. The idea that teachers are unfair to students is nonsense.	Most students don't realize the extent to which their grades are influenced by accidental happenings.

---

A	B
7. Without the right breaks one cannot be an effective leader.	Capable people who fail to become leaders have not taken advantage of their opportunities.

---



---

A	B
8. No matter how hard you try some people just don't like you.	People who can't get others to like them don't understand how to get along with others.

---

A	B
9. Heredity plays the major role in determining one's personality.	It is one's experiences in life which determine what they're like.

---

A	B
10. I have often found that what is going to happen will happen.	Trusting to fate has never turned out as well for me as making a decision to take a definite course of action.

---

A	B
11. In the case of the well prepared student there is rarely if ever such a thing as an unfair test.	Many times exam questions tend to be so unrelated to course work that studying is really useless.

---

A	B
12. Becoming a success is a matter of hard work, luck has little or nothing to do with it.	Getting a good job depends mainly on being in the right place at the right time.

---

A	B
13. The average citizen can have an influence in government decisions.	This world is run by the few people in power, and there is not much the little guy can do about it.

---

A	B
14. Knowing the right people is important in deciding whether a person will get ahead.	People will get ahead in life if they have the goods and do a good job; knowing the right people has nothing to do with it.

---



- 
- | A   | B  |
|---|--|
| 15. When I make plans, I am almost certain that I can make them work. | It is not always wise to plan too far ahead because many things turn out to be a matter of good or bad fortune anyhow. |
- 
- | A  | B                                |
|--|----------------------------------|
| 16. There are certain people who are just no good. | There is some good in everybody. |
- 
- | A   | B  |
|---|--|
| 17. In my case getting what I want has little or nothing to do with luck. | Many times we might just as well decide what to do by flipping a coin. |
- 
- | A   | B  |
|---|--|
| 18. Who gets to be the boss often depends on who was lucky enough to be in the right place first. | Getting people to do the right thing depends upon ability, luck has little or nothing to do with it. |
- 
- | A   | B   |
|---|---|
| 19. As far as world affairs are concerned, most of us are the victims of forces we can neither understand, nor control. | By taking an active part in political and social affairs the people can control world events. |
- 
- | A  | B  |
|--|--|
| 20. Most people don't realize the extent to which their lives are controlled by accidental happenings. | There really is no such thing as "luck." |
- 
- | A   | B  |
|---|--|
| 21. One should always be willing to admit mistakes. | It is usually best to cover up one's mistakes. |
-





---

A	B
22. It is hard to know whether or not a person really likes you.	How many friends you have depends upon how nice a person you are.

---

A	B
23. Leadership positions tend to go to capable people who deserve being chosen.	It's hard to know why some people get leadership positions and others don't; ability doesn't seem to be the important factor.

---

A	B
24. In the long run the bad things that happen to us are balanced by the good ones.	Most misfortunes are the result of lack of ability, ignorance, laziness, or all three.

---

A	B
25. With enough effort we can wipe out political corruption.	It is difficult for people to have much control over the things politicians do in office.

---

A	B
26. Sometimes I can't understand how teachers arrive at the grades they give.	There is a direct connection between how hard I study and the grades I get.

---

A	B
27. A good leader expects people to decide for themselves what they should do.	A good leader makes it clear to everybody what their jobs are.

---

A	B
28. Many times I feel that I have little influence over the things that happen to me.	It is impossible for me to believe that chance or luck plays an important role in my life.

---



- 
- | A   | B   |
|---|---|
| 29. People who don't do well in life often work hard, but the breaks just don't come their way. | Some people just don't use the breaks that come their way. If they don't do well, it's their own fault. |
- 
- | A  | B  |
|--|--|
| 30. People are lonely because they don't try to be friendly. | There's not much use in trying too hard to please people, if they like you, they like you. |
- 
- | A   | B  |
|---|--|
| 31. There is too much emphasis on athletics in high school. | Team sports are an excellent way to build character. |
- 
- | A                                       | B   |
|---|---|
| 32. What happens to me is my own doing. | Sometimes I feel that I don't have enough control over the direction my life is taking. |
- 
- | A   | B  |
|---|--|
| 33. Most of the time I can't understand why politicians behave the way they do. | In the long run the people are responsible for bad government on a national as well as on a local level. |
-



## APPENDIX G

The top figure indicates means for all 64 Ss; the bottom figure indicates means for the 34 Ss for which I-E scores were available. For the content of the dependent variable items #1-11, see Appendices B and C; D. V. #12 is the summed justification score.

<u>D. V. #1</u>	<u>D. V. #2</u>	<u>D. V. #3</u>	<u>D. V. #4</u>
4.72	4.42	4.34	4.66
4.62	4.29	4.18	4.53
<u>D. V. #5</u>	<u>D. V. #6</u>	<u>D. V. #7</u>	<u>D. V. #8</u>
2.09	3.45	4.92	3.11
1.97	3.39	4.94	2.94
<u>D. V. #9</u>	<u>D. V. #10</u>	<u>D. V. #11</u>	<u>D. V. #12</u>
3.13	1.84	2.13	17.86
3.18	1.85	2.18	17.74

Means for the 34 Ss for which I-E data were available:

<u>Rotter I-E score</u>	<u>Personal factor</u>
10.81	4.29
<u>Political factor</u>	<u>Ideological factor</u>
2.91	3.00

For D. V.'s 1-12, t-tests for differences between means (Hays, 1963) revealed no significant differences between means for the entire sample and means for the 34 Ss for which I-E data were available.











